south to south-west prevail, which are explained by the distribution of the isobars.

Another campaign from July to October, 1907, gave the same general results, as well for the direction of the atmospheric currents as for the vertical distribution of temperature (Figs. 2 and 4). This voyage was not continued further south than 10° N., but the vessel remained twice during twelve days near this parallel, this latitude having been selected on account of the regularity of the trade wind. Again the easterly current predominated at all heights, and also the layer of inverted temperature near 1000 metres.

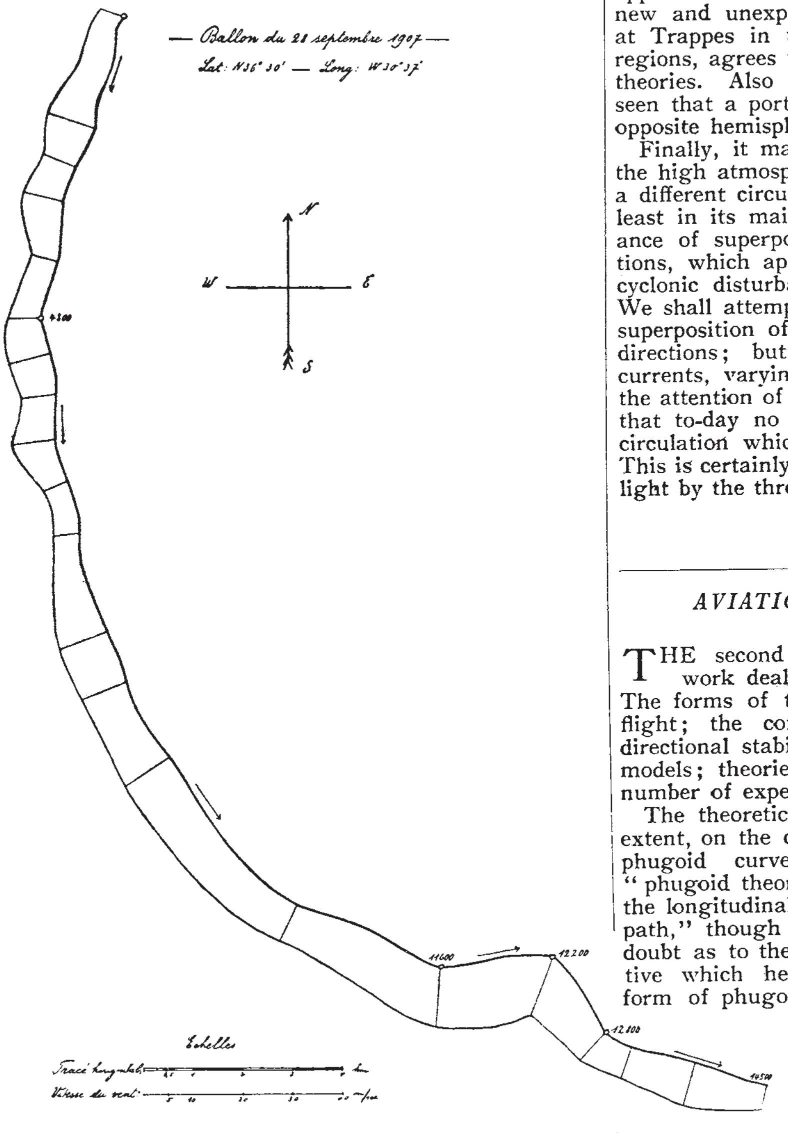

FIG. 4.—Motion of air in the atmosphere in the region of the Azores.

Note.—In all the figures the numbers give the altitude and the breadth of the lines the velocity of the wind.

The ***ballon-sonde*** ascensions made in 1907 were somewhat higher than before, and the isothermal layer was reached at 14 kilometres in latitude 25° 18′ N. North of 25° the isothermal layer was often met with at altitudes varying from 12 to 14 kilometres, while to the south of this parallel it was not reached, although the balloons many times exceeded 15 kilometres. Therefore, it appears that the altitude at which the temperature ceases to decrease is much greater near the equator than in moderate latitudes. This distribution of temperature is also confirmed for the regions of the Azores and Canaries, as far as about 25° N., by the ascensions of *ballons-sondes* made by Prof. Hergesell.

Our conclusions concerning the direction of the upper currents correspond in substance with former ideas about the anti-trades, apart from the situations which produce currents of very different and almost opposed directions lying one above the other. This new and unexpected fact, which was also observed at Trappes in the barometric maxima of our own regions, agrees tolerably well with certain of Maury's theories. Also near the equator it can be distinctly seen that a portion of the anti-trade comes from the opposite hemisphere.

Finally, it may be said that, if the exploration of the high atmosphere over the Atlantic does not show a different circulation from that already supposed, at least in its main features, it emphasises the importance of superposed strata flowing in various directions, which appear to persist in the regions where cyclonic disturbances of large diameter rarely form. We shall attempt later to give an explanation of the superposition of two or three strata having different directions; but the stratification of numerous thin currents, varying in their motions, is a fact worthy the attention of meteorologists, for we must recognise that to-day no theory explains this special mode of circulation which extends over a very large region. This is certainly one of the important facts brought to light by the three cruises of the *Otaria*.

L. TEISSERENC DE BORT.
A. LAWRENCE ROTCH.

AVIATION, MATHEMATICAL AND OTHERWISE.[1]

THE second volume of Mr. Lanchester's large work deals mainly with the following points:—The forms of the paths described by bodies in free flight; the conditions of longitudinal, lateral, and directional stability; the theory and use of scale-models; theories of soaring flight; and a large number of experimental verifications.

The theoretical discussions are based, to a large extent, on the consideration of what the author calls phugoid curves. According to the "Glossary," "phugoid theory" means "the theory dealing with the longitudinal stability and the form of the flight path," though in a footnote the author raises some doubt as to the appropriateness of the Greek derivative which he has himself coined. The simplest form of phugoid curve, to the study of which the author devotes considerable attention, might form the subject of problems that would delight the heart of the old-fashioned tripos examiner. Like the latter's particle on his perfectly smooth surface, the gliding body is supposed to have its mass concentrated at a single point and to travel without loss of energy, and the supporting surface is supposed to be small and to be always tangential to the direction of motion. In other words, the problem reduces to that of a particle acted on by gravity and by a supporting force (due to the air) which is always normal to the direction of motion and proportional to the square of the velocity.

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[1] (1) "Aërodonetics." Constituting the Second Volume of a complete Work on Aërial Flight. By F. W. Lanchester. Pp. xvi+433. (London: A. Constable and Co., Ltd, 1908.) Price 21*s.* net.
(2) "Artificial and Natural Flight." By Sir Hiram S. Maxim. Pp. xv+166. (London: Whittaker and Co., 1908.) Price 5*s.* net.

In view of the fact that classes on calculus for engineers form an integral part of every modern technical course of instruction, it is to be regretted that Mr. Lanchester has not made some attempt to bring his equations more into conformity with ordinary well-recognised notation. When he writes down the equation of his curve as

$$\cos \Theta = \frac{\mathrm{H}}{3\mathrm{H}_n} + \frac{\mathrm{C}}{\sqrt{\mathrm{H}}},$$

and the expression for the radius of curvature as

$$r = \frac{d\mathrm{L}}{d\Theta},$$

his readers will take some time to find out what these equations mean; whereas any student who has attended the classes above referred to would understand at a glance the same equations if written as

$$\frac{dx}{ds} = \frac{y}{3v_0} + \frac{c}{\sqrt{y}} \text{ and } \rho = \frac{ds}{d\phi}.$$

When Mr. Lanchester applies his phugoid theory to investigate the longitudinal stability of aërofoils, he at once comes into conflict with the theory which the present writer, in conjunction with Mr. Williams, worked out some few years ago. There has been some difficulty in making out how Mr. Lanchester arrives at his results, and Mr. Harper has examined the matter independently. It was not intended at first to deal with what might be a controversial point in a review in Nature, but the difference between the two methods is probably not so very difficult to explain.

According to our theory, the small oscillations about steady motion of an aëroplane, or indeed any body moving in a resisting medium in a vertical plane, depend on the roots of a biquadratic equation, and the conditions for stability are those given by Routh. This method enables account to be taken of every circumstance which may affect the stability, in particular, variations in the position of the centre of pressure for different angles.

Mr. Lanchester, on the other hand, considers only the case of a single aëroplane, the variations of the centre of pressure of which are not taken into account, stability being maintained by means of a tail. He starts with the assumption that his "phugoid" oscillations, when small, are simple harmonic in character, and that the effect of the moment of inertia of the machine, as well as of resistances, is to change the amplitude of these oscillations. In estimating the effect of these changes he assumes the equations of simple harmonic motion to hold good. For example, in considering the rotatory motion about the centre of gravity (§ 63) he writes

$$\tau_1 = \frac{2\mathrm{I}\pi^2}{t_1^2} \times 2\Theta_1 = \frac{4\Theta_1 \mathrm{I}\pi^2}{t_1^2},$$

where τ_1 is the maximum torque, Θ_1 the maximum angular displacement, t_1 the time of oscillation (calculated, it would seem, from the ideal "phugoid" motion), and I the moment of inertia.

This step is unjustifiable. The correct equation is

$$\mathrm{I}\frac{d^2\theta}{dt^2} = \tau,$$

where τ is the torque at any instant. A similar consideration applies higher up, and when the necessary corrections have been made they are found to lead to the biquadratic equation of the Bryan-Williams theory.

It is as if, in working out the theory of the compound pendulum, it were attempted to treat one weight as a simple pendulum, and to assume that the motion of the other weight did not affect the period, or the relation between velocity and displacement, but merely produced variations of amplitude.

Mr. Harper has applied the Bryan-Williams method to the particular kind of tailed aëroplane considered by Mr. Lanchester, and he obtains a numerically different result, the discrepancy being accountable for by the assumptions contained in Mr. Lanchester's method.

There is thus a good bit of work of a theoretical character requiring to be done before the problem of stability can be regarded as completely solved. In the meantime, it must be remembered that airship designers have not, as a rule, undergone even the elementary training in practical mathematics referred to in this review, and that most extraordinary views commonly prevail in this country regarding the subject of stability. It is not improbable that Mr. Lanchester's conditions may be sufficiently near the mark for practical purposes, and his experimental verifications seem to support this view. Moreover, they may err on the side of safety. It seems also certain that unstable machines have been safely guided through the air by skilled manipulators, and the stability of the Wright machine has been seriously questioned. Indeed, there are good reasons for believing it to be unstable. Mr. Lanchester's method applied to the Lilienthal machine shows it to be unstable, although, in view of its broad curved supporting surfaces, a complete investigation would require account to be taken of several neglected factors for which no experimental data exist.

A great deal of rubbish has often been written on the "soaring bird," and much that has been stated in print has been incompatible with the doctrine that perpetual motion is impossible. Mr. Lanchester's observations and experiments are deserving of the most careful consideration, and the same applies to his chapter on "Experimental Aërodonetics." The book represents the result of a serious effort to place the theory of flight on a scientific basis, and should do much to convince would-be aviators that "airship design" can no longer be regarded, as it has been in the past, as a mere exercise for the imaginative faculty, but as a subject requiring hard thought, endless experiments, and great care in drawing conclusions from them.

Sir Hiram Maxim's book is distinctly disappointing. An account of his early experiments, if somewhat out of date, would be at least of historic interest; but when the author indulges in a tirade against mathematicians, the question which naturally suggests itself is, Where on earth does he find his mathematicians? He tells us that

"During the last few years a considerable number of text-books have been published. These have, for the most part, been prepared by professional mathematicians who have led themselves to believe that all problems connected with mundane life are susceptible of solution by the use of mathematical formulæ, provided, of course, that the number of characters employed are numerous enough."

Now Prof. Chatley, who certainly has got a pretty clear grasp of the present state of the flight problem, recently wrote:—

"A few great mathematicians (including Lords Kelvin and Rayleigh) have devoted some attention to the matter, but the author is not aware that any mathematician worthy of the name has considered it worth while to make an exhaustive study of the question. . . ."

When Sir Hiram says that "Up to twenty years ago Newton's erroneous law as relates to atmospheric resistance was implicitly relied upon, and it was not the mathematician who detected its error, in fact we have plenty of mathematicians to-day who can prove by formulæ that Newton's law is absolutely correct and unassailable. . . ." his information does not agree with the facts of the case. What about Kirchhoff's theory of discontinuous fluid motion, to mention nothing else?

Again, it is rather amusing to see mathematicians accused of demonstrating "by formulæ, unsupported by facts, that there is a considerable amount of skin-friction to be considered," when the usual complaint is that they will assume all their bodies to be perfectly smooth, and will not take account of frictional resistances in solving their problems. But some clue as to where Sir Hiram finds his mathematicians is afforded by his reference (preface, p. x) to a recent controversy in *Engineering*. Surely he cannot suppose that the authors of difficult mathematical researches would, as a rule, publish their best work in journals devoted to the interests of practical engineers, even if the editors would consent to print them! If he would consult the pages of journals and transactions devoted to researches in mathematics and mathematical physics, he would soon discover the paucity of papers to which Prof. Chatley refers.

On pp. 104 to 108 he publishes figures of stream-lines taken (so he says) from "mathematical" treatises, and all he is able to say is that "just how or why" the air moves in these particular ways is not evident. Now, in the first place, the diagrams show complex systems of eddies, the equations of motion of which no mathematician would ever attempt to integrate, and in the second place the question is not how the air is *likely* to move, but how it actually does move?

As an exponent of experimental *versus* mathematical methods, why did not Sir Hiram put the matter to a decisive and conclusive test by determining experimentally the form of the stream-lines produced in the neighbourhood of the various surfaces shown in these illustrations? Experimental, and in particular photographic, methods of plotting stream-lines are not difficult, and they can be conducted at a very trifling expense. Some of those who are, or have been, conducting such experiments are not altogether unmathematical in their methods. Surely Sir Hiram Maxim has missed a grand opportunity of scoring off his "mathematicians."

It was in 1894 that the author's gigantic experimental machine ran to and fro between rails. To all that has been done since that time only about five pages, including illustrations, are devoted in a chapter on "Some Recent Machines," and an equal number in a chapter headed "Balloons"; and yet the fifteen years that have just elapsed form the most eventful period in the whole history of artificial flight. It is the experimenters who have expended time and money, and have even sacrificed their lives, rather than the mathematicians, who have cause for disappointment at the scanty recognition they have received.

An address on "Recent Progress in Aëronautics," delivered before the engineering section of the American Association at Baltimore by Major George O. Squier, is published in *Science* for February 19. It is in the nature of a general summary, and deals both with balloons and aëroplanes, but the treatment of resistances on "arched surfaces" reveals an important gap in the experimental information dealt with in the address. It is tacitly assumed that the only effect of arching the surface is to increase the coefficient of resistance, the angle of flight being taken "to be the inclination of the chord of the surface to the line of translation." This would be all right if we were sure that the resultant reaction was always perpendicular to the chord, but it is pretty certain that such is not the case. If the aërocurve forms a circular arc, the resultant must (in the absence of skin-friction) pass through the centre of curvature, and if the centre of pressure is in front of the centre of the arc, the effective angle of flight will be less than the inclination of the chord, that is, the ratio of drift to lift will be less than the tangent of the inclination of the chord. Experimental information on this point is very scanty as a rule, a notable exception being Mr. Turnbull's investigations of plane, concave, convex, and doubly curved surfaces. Again, exception may be taken to the statement that "the helicopter type of machine may be considered as the limit of the aëroplane when by constantly increasing the speed the area of the supporting surfaces is continuously reduced until it practically disappears."

In his suggestions for "the stabilisation of aëroplanes" in *La Revue des Idées* (Paris, February 15), M. Étienne Maigre deals with lateral stability, and assumes that the lateral balance is to be maintained, not automatically, but by the voluntary or involuntary effort of the aviator. He suggests the use of two triangular surfaces attached to the main aëroplane and controlled by hand. He assumes Otto G. Luyties' law, according to which the normal resistance varies as $2 \sin a - \sin^2 a$, and finds a maximum lift for an angle of 37°.

Captain Renard has been giving a series of conferences before the Société d'Encouragement pour l'Industrie national, of which the first has appeared in the *Bulletin* (Paris, January). Captain Renard distinguishes six different methods of experimenting on air-resistance, including the use of experiments in water, with suitable allowances for difference of density. The need of further experiments in this direction is strongly emphasised.

It is to be noticed that the art of designing gigantic airships fitted with saloons, cabins, and mess-rooms has not yet faded away into past history, despite the recent advances in aëroplanes and dirigibles. About the beginning of February the *Standard* devoted more than half a column to an American project very suggestive of the *Minerva* of Robertson or the gigantic apparatus for which M. Petin raised 1000*l*. in the early days of ballooning, but for which the gas supply proved inadequate.

G. H. Bryan.

DEW-PONDS.

A GROWING interest in the subject of dew-ponds has been exhibited in recent years, but it has yet to be proved whether there is actually such a thing as a true dew-pond. Dew-and-mist ponds there undoubtedly are, but dew and mist, similar in essence as they may be, are yet distinct and separate meteorological phenomena. The term "dew-pond" has arisen from the careless habit of assuming that every form of condensation of aqueous vapour which is not seen as rain or snow must be called dew.

The Journal of the Society of Arts (March 5) contains a paper on the subject by Mr. Geo. Hubbard, and he has therein endeavoured to show how artificial deposition of dew may be brought about in a pond. He maintains that by laying down a bed of straw beneath puddled clay, the water may be chilled sufficiently to cause the atmosphere to give out aqueous vapour as dew. In his earlier remarks on the subject, it was the chilling of the puddled clay to which he attributed deposit of dew. Of course, if a pond were fairly full there would be but little puddled clay exposed, so now he attributes the additional supply to